Geosci. Model Dev. Discuss.,
https://doi.org/10.5194/gmd-2018-22-RC1, 2018

Geoscientific Model Development Discussions

Interactive comment on "Implementation of a simple thermodynamic sea ice scheme, SICE version 1.0-38h1, within the ALADIN-HIRLAM numerical weather prediction system version 38h1" *by* Yurii Batrak et al.

Anonymous Referee #1

Received and published: 23 April 2018

1 General comments

The paper presents results for the implementation of a simple sea ice thermodynamic model in a numerical weather prediction forecasting system. This is the first time such a sea ice model has been implemented in this particular NWP system, and a paper on the implementation and results should thus in principle merit publication.

However, I have serious concerns about the use of a constant ice thickness, about

the limited nature of the results used for validation of the model (covering only a short period in March-April 2013), and about some of the methods used for analysis. Some of the language used is also quite cumbersome and difficult to follow.

I can therefore recommend publication only after major revisions, which I outline below.

2 Specific comments

As mentioned above, I thought some of the language used was quite cumbersome and difficult to follow. I would strongly advise the authors to ask a native English speaker to proof-read the paper before resubmitting.

The authors refer in the introduction to the Met Office Unified Model (page 2, lines 19-20), and state that "...to our knowledge there are no publications about the details of coupling between the advanced sea ice model and the atmospheric model in this system". This is incorrect. The model setup for coupled NWP is described by Lea et al. (2015), while the coupling is described in detail by Hewitt et al. (2011). The authors should cite both of these papers.

On page 2, lines 15-18, the authors state that advanced sea ice models are "applied ... in coupled ocean-ice-atmosphere systems for research purposes and seasonal forecasting". I would suggest that they also mention that such sea ice models are used in coupled climate models, such as HadGEM3 (Hewitt et al., 2011).

The description of the model appears to be split between Sections 1, 2 and 3. However, I would prefer to see one single model description section. This could be done in an expanded Section 2. In the current version of the paper, there is some description of the model setup in lines 5-27 of page 3 in the introduction. The authors then describe the sea ice parametrization scheme itself in Section 2. Then, within Section 3, Section 3.2 describes the experimental configuration. I appreciate that some level of discussion of the model is needed in the introduction, but I think the authors probably go into too much detail here. The introduction should set the scene, describe briefly the scientific background and work done by previous authors, and how the present paper builds on that. Much of the discussion of the model would more properly belong in a model description section. Similarly, Section 3 is primarily a results section. I think the description of the experimental configuration in Section 3.2 would again be better placed in an expanded Section 2.

The model is run with a constant ice thickness of 0.75m. However, the use of a constant thickness is likely to lead to be a source of considerable uncertainty, as ice surface temperature will be extremely sensitive to thickness. Indeed, the authors state in Section 3.1 that "when the ice thicknesses [in SICE and HIGHTSI] are very different, the ice surface temperature values may differ by more than 5°C", and in the conclusions, they say "the sensitivity of the results to the prescribed value of the ice thickness was noticed". Later, they also say that "the simplest way to go forward is to replace the prescribed constant value of the ice thickness by the climatology, to reproduce its seasonal and horizontal large scale variations". This begs the question of why the authors didn't use such a climatology in the current paper. In a resubmitted version of the paper, I would like to see results from a configuration of the model in which the local thickness in each gridbox is prescribed from climatology.

I am confused by Section 3.1. The authors state that they only compared SICE with HIGHTSI where the ice thickness in HIGHTSI was "approximately equal" to the constant thickness used in SICE (0.75m). However, they then state that they consider "small" ice thickness differences to be less than 0.4m, which is more than 50% of 0.75m; I would not say that such thicknesses are "approximately equal" to each other. The authors then state "When the ice thicknesses are very different, the ice surface temperature value may differ by more than 5°C", which suggests that, contrary to their previous statement, they have in fact analysed the results for larger differences in ice thickness between the two models. They also do not state what they mean by "very

different" - do they mean the difference is greater than 0.4m?

In Section 3.2 (page 10, line 16), the authors mention that the model is "started from the snow-free state and allowed to accumulate snow from precipitation during the modelling period". What impact will this have on the forecasts? How long will SICE2D-S take to "spin up" to a realistic representation of the snow cover? It will surely be much longer than the 48 hours of the forecasts in the current paper. For this reason, I am not sure how much weight we can give to the SICE2D-S results. The authors should at least comment on this in the paper, and should preferably present results for SICE2D-S forecasts that have been started from a spun-up snow state.

In Sections 3.3 and 3.4, the authors analyse forecasts only for a short period (March-April 2013). However, the performance of the model is likely to vary during the year, and the results may well be different in different seasons. I note that the authors state in the conclusions that in the future the model will be evaluated for more regions and more seasons, but I am of the opinion that results for other seasons must be included in the present paper for it to be worthy of publication. Given that Arctic sea ice exhibits a clear annual cycle, it is important that the impact of this on the performance of the model is analysed.

In Figures 2, 3 and 6, the authors present results for the mean error in the forecast MSLP, 2-metre temperature, and 10-metre wind speed, where the error is defined as the difference between the modelled and observed quantities, and the mean is taken over several observing sites. However, the standard deviations plotted in Figures 2, 3 and 6 are often much larger than the differences between the means. The authors discuss the differences between the mean errors for different experiments at length in Section 3.3, and consider possible reasons for them, but the fact that the standard deviations are so large compared to the differences suggests that the differences may not be significant. If this is indeed the case, then it suggests that the SICE scheme, and the related snow and form drag schemes, may not have a significant impact on the model-obs errors. It would be interesting to see if this conclusion changes when the authors use RMS error rather than simple mean error, and when they look at forecasts for different times of year.

Another point relating to Figures 2, 3 and 6 is that the use of a simple mean error will potentially lead to positive and negative errors cancelling each other out. This will hide potentially-relevant results if some stations have a very large positive bias and others have a large negative bias. For this reason, I think the root-mean-square error would be a more useful quantity to assess, and I would like to see a plot of this, either instead of or in addition to the simple mean error that the authors have plotted here.

It would also be interesting to see the contributions of the different observing stations to the mean (or RMS) error. This could be done using maps of a similar form to Figure 3 of Bellouin et al. (2011), where the observations are shown with boxes superimposed on a map showing the fields output by the model.

The authors mention in the text (page 10, line 21) that they used 12 Svalbard stations, and indeed 12 are shown on the map in Figure 1. However, in the captions of Figures 2 and 6, they mention "7 Svalbard stations", and list the 7 stations. I presume this is because of the issues described in Section 3.3 (page 10, lines 23-26) whereby some Svalbard stations were excluded because they were in fjords. But were the other 5 stations used at all in this analysis? If not, then it is incorrect to state at the beginning of Section 3.3 that measurements from 12 Svalbard stations were used (as in fact only 7 were used). The authors should re-word this paragraph (page 10, lines 21-26) to make this clearer.

In Section 3.3 (page 11, lines 14-18), the authors discuss the relative sizes of the standard deviations of the errors in REF and SICE2D-NS, without any mention of the implications or relevance of these results. Presumably a smaller standard deviation implies that there is a smaller range of errors between stations. Is this relevant, and if so why? Is there any indication what might be causing it? Does the implementation of the sea ice scheme affect the 2-metre temperature at some stations more than others?

Is there an obvious reason for this?

At the end of Section 3.3 (page 12, lines 31-34), the authors state "...with observations from coastal stations only, we lack understanding of the ice temperature behaviour for larger scales". This is a very good point to make, and I would recommend that when the authors resubmit the paper they include results for a wider range of stations within the forecast domain, including non-coastal (i.e. inland) stations. Does the implementation of the sea ice scheme affect the results only at stations that are physically close to the sea ice, or are there larger-scale effects?

Figure 7 shows surface temperature derived from MODIS data, and forecast by the model. However, it is quite difficult to get an idea of the differences between the temperature fields in the plots. It would be much more useful if the authors could present maps showing the difference between these (i.e., model minus MODIS). This would help the reader to understand better the results discussed in Section 3.4.

The authors mention in Section 3.4 (page 13, lines 10-12) that most MODIS swaths were in the daytime. However, the model results shown in Figure 7 are whole-day averages. How will this affect the comparison of the two? I imagine there may be a warm bias in the MODIS observations as a result of the fact that they are generally restricted to daytime. The authors should comment on this, and its implications for the results, in the paper.

3 Technical corrections

- Page 2, lines 2-3: "Over areas with a mixture of floes and polynyas, the form drag appears, which affects the turbulent fluxes". I would suggest re-wording this, so that it reads: "Over areas with a mixture of floes and polynyas, the turbulent fluxes are affected by form drag".

- Page 2, lines 19 and 34: I don't like the use of "To our knowledge...", as it seems unscientific to me. I have already mentioned above that the statement made on lines 19-20 is in fact incorrect. I would also suggest an alternative wording for the sentence on lines 34-35. Indeed, if one doesn't know whether a particular statement is true or not, it is often best not to include it at all, rather than preceding it with "To our knowledge...".
- Page 4, line 6: "...it is designed so that it can be naturally coupled with a snow scheme...": I don't know what the authors mean by "naturally coupled". I think that "...so that it can be coupled to a snow scheme..." would suffice.
- Page 7, line 25: "It is important to mention that...": This is unnecessary. If it's important to mention it, then mention it – there is no need to say that it's important to do so.
- Page 9, line 25: "The background for the data assimilation are fields of prognostic variables...": I think there is a word missing here, and that this should read " The background fields for the data assimilation...".
- Page 10, line 27: Figure 6 is mentioned before Figures 4 and 5. The figures should be re-ordered to avoid this.
- Page 11, lines 6-7: "...the underestimation of night-time 2 metre temperatures over land is a characteristic feature of the model known from operational verification (not shown)". If this is known from operational verification, is there a reference that the authors can cite?
- Page 11, lines 14-15: "The error standard deviation for the 2 metre temperature forecasts...". This should read "The standard deviation of the errors in the 2 metre temperature forecasts...".

- Page 11, line 22: "mean sea level pressure error standard deviation" sounds clumsy. I would suggest "standard deviation of the error in mean sea level pressure". The authors could also abbreviate "mean sea level pressure" to "MSLP", if they define the abbreviation the first time they use it.
- Page 11, line 31: " over the part of the grid cell related to the sea with ice". I'm not sure what this means. Does it mean "over the ice-covered part of the grid cell", or something else? I would suggest re-wording this to make it clearer.
- Page 12, lines 4 and 18: I think the authors mean "in agreement with" rather than "in accordance with".
- Page 12, line 20: "...makes the surface temperature drop more and more". This language ("more and more") is not very scientific. Please consider re-wording.
- Page 14, line 18: "...the sensitivity of the results to the prescribed value of the ice thickness was noticed". I think the authors mean "noted" rather than "noticed".
- In the caption of Figure 3, the authors mention 7 stations in the Gulf of Bothnia, and 7 are shown in the map in Figure 1, but in the text (page 10, line 21) they state that they used 6 stations in the Gulf of Bothnia.
- The authors state in the text that the modelled ice surface temperature shown in Figure 7 is for the configuration which doesn't include the snow scheme (i.e., SICE2D-NS), but it would be helpful to the reader if they also re-stated this in the figure caption.

4 References

- Bellouin, N., J. Rae, A. Jones, C. Johnson, J. Haywood, and O. Boucher, 2011: Aerosol forcing in the Climate Model Intercomparison Project (CMIP5) simulations by HadGEM2âĂŘES and the role of ammonium nitrate, *J. Geophys. Res.*, **116**, D20206, doi:10.1029/2011JD016074.

- Hewitt, H. T., D. Copsey, I. D. Culverwell, C. M. Harris, R. S. R. Hill, A. B. Keen, A. J. McLaren, and E. C. Hunke, 2011: Design and implementation of the infrastructure of HadGEM3: The next-generation Met Office climate modelling system. *Geosci. Model Dev.*, **4**, 223–253, doi:10.5194/gmd-4-223-2011.
- Lea, D. J., I. Mirouze, M. J. Martin, R. R. King, A. Hines, D. Walters, and M. Thurlow, 2015: Assessing a New Coupled Data Assimilation System Based on the Met Office Coupled Atmosphere–Land–Ocean–Sea Ice Model. *Mon. Wea. Rev.*, **143**, 4678âĂŤ4694, doi:10.1175/MWR-D-15-0174.1.

Interactive comment on Geosci. Model Dev. Discuss., https://doi.org/10.5194/gmd-2018-22, 2018.